ESSAY REVIEW

The War on Newton

By Mordechai Feingold*

J. B. Shank. *The Newton Wars and the Beginning of the Enlightenment*. xv + 571 pp., illus., bibl., index. Chicago/London: University of Chicago Press, 2008.

To J. B. Shank, the engrained notion that Newton's genius can account for the advent of scientific modernity and the subsequent French Enlightenment is seriously misguided. Though conceding the centrality of Newtonianism to the *philosophe* movement, Shank seeks to root out the "myth" that Newtonianism served as the "natural springboard" for the Enlightenment. He tethers his narrative to the self-serving *philosophes*, whose version of events was accepted *de facto* and then perpetuated by generations of scholars. Newton's critical role, Shank asserts, was historically contingent, becoming effective only after the publication in 1734 of Voltaire's *Lettres philosophiques*. Shank accordingly devotes the first part of *The Newton Wars and the Beginning of the Enlightenment* to establishing the context of Voltaire's work: the new public culture in France that reshaped the Republic of Letters during the early eighteenth century. Seminal to this transformation, he contends, were structural shifts in French institutional and public science, including the proliferation of new journals; the emergence of a critical spirit that replaced the civility that had hitherto informed transactions in the Republic of Letters; and the rise of a particularly aggressive strain of Newtonianism—with the concomitant coalescence of a hostile Cartesian party. This new cultural landscape enabled Voltaire to articulate the identity of the *philosophe*, and it was this novel identity that then launched the Enlightenment.

The second part of *The Newton Wars* seeks to instantiate the argument through a detailed analysis of the personae and literary productions of Voltaire and Maupertuis in the context of the public rows they provoked. Such analysis is in line with Shank's thesis that philosophical ideas—in and of themselves—were marginal to the emergence of the French Enlightenment; what mattered was the manner in which such ideas were deployed by Voltaire and "the particular self-fashioning he accomplished with them, a self-fashioning that led to the definition of a new kind of critical, libertarian intellectual in France" (p. 31). Maupertuis exhibited a complementary model. Whereas Voltaire operated exclusively within the domain of the public sphere, Maupertuis's base was initially the Académie des Sciences, which he transformed, then supplemented, with his own public sociability. This sociability included the same markings of libertinism and philosophical

* Division of Humanities and Social Sciences, California Institute of Technology, Pasadena, California 91125.

Isis, 2010, 101:175–186

0021-1753/2010/10101-0011$10.00

radicalism that informed the early Voltaire and would become the distinctive trademark of Diderot.

As for Newtonianism, it provided both Voltaire and Maupertuis with the means to establish their respective public identities and styles of argumentation, as well as to promote their (occasionally interlocking) agendas. Newtonianism, in other words, is conceived of by Shank as a "discourse"—in sync with his explicit acknowledgment of intellectual debts to Michel Foucault and, to a lesser degree, Pierre Bourdieu, Bruno Latour, and Edward Said. Not surprisingly, Shank applies a distinctly postmodernist scaffolding to Newton's deification by the *philosophes* and his intellectual dominance during the second half of the eighteenth century. As he explains in a stylistically typical passage, these key developments

> were not natural outcomes of a progressive and teleological Enlightenment modernity, but, rather, a set of particular historical outcomes produced by French men and women caught up in a complex web of temporal, spatial, and other local contingencies. In short, this is a book about a beginning that seeks to escape the spell of teleological origin stories. It is also a history of Newton and Enlightenment in France that self-consciously detaches itself from the living history that continually naturalizes their marriage as a self-evident feature of modernity. To state the same point another way, this is a postmodern and post-Enlightenment history of one crucial moment in the beginning of Enlightenment modernity, the moment when Newtonianism became linked to it as its genetic code and avatar. [Pp. 14–15]

Shank further promotes his book as "a work of Foucault-inspired revisionist historical scholarship, one that marshals extensive and carefully analyzed documentary evidence in support of a new and different genealogical understanding of the Newtonian legacy in eighteenth-century France" (p. 25). Accordingly, the first part of *The Newton Wars* aims to rid us of two misguided beliefs: that Newton played a "Promethean role in the birth of scientific modernity" (p. 22) and that there existed "an intransigent war between Newton and Descartes [that proved to be] the dominant theme of Newton's French reception" (p. 19). Quite the contrary: "France absorbed Newton's science immediately and in substantial, if idiosyncratic, ways from as early as 1690" (p. 28).

Shank's thesis rests on the assumption that a vibrant tradition of *mécanique* existed in France, the followers of which developed a distinctively French algorithmic science of motion, to which was then grafted elements of Newton's science. Since this native analytical mechanics derived its inspiration from Malebranche's mathematical phenomenalism, not from Newton, the battles over the calculus and related issues in France had little, if anything, to do with the Englishman. To this I take strong exception. To characterize French analytical mechanics as virtually devoid of clear and demonstrative connections to the *Principia* grossly exaggerates, if not downright distorts. The work of Niccolò Guicciardini, and even that of Varignon's major modern proponent, Michel Blay, demonstrates that the fundamental conceptual approach undergirding French analytical mechanics derived from careful consideration of concepts and methods set forth in Newton's masterpiece. Varignon, for example, had begun as early as 1695 to consider the issues posed for analysis by central force motion—the very concept that Newton had developed. Varignon's own contribution consisted in the systematization of results already known and in the Leibnizian reformulation of central parts of the *Principia* itself. True, one area that Newton treated with indifferent success—the motion of bodies in resisting media—did attract special interest and novel development on the Continent. Yet the

question of the foundation of what became analytical mechanics has long been a complex issue, requiring a subtler historical touch than is apparent in *The Newton Wars*.[1]

Effectively, Shank proposes to split the mathematics of the *Principia* from its underlying physical conceptions in respect to force. But the mathematics of force cannot simply be separated from the *Principia*'s novel approach to the connection between force and motion. Nor is this connection altogether coincident with a metaphysics according to which "force" need not be transported by a material object. Even so, the *Principia*'s methods for working the force–motion connection were sufficiently novel to generate bewilderment when first encountered, even by such luminaries as Huygens and Leibniz. Shank, however, elides this difference, thereby erasing the considerable resistance that the *Principia*'s novel mathematics for linking force to motion encountered.[2]

Lack of precision in explicating the actual content of the period's science is equally evident in Shank's remark concerning the physics of Newton's *Opticks*. Shank claims that Newton had argued that light, composed of material corpuscles, was "subject to a law of universal gravitational attraction" (p. 114). He should have said that the Newtonian particles of light are subject to forces that are distinctly and notably different from gravity in their mode of operation—that, in fact, these forces, unlike gravity, could have nothing directly to do with the masses of the particles in question, thereby raising difficult problems that were often discussed during the eighteenth century.

Shank's doubtful representation of analytical mechanics is further undercut by his contention that it constituted the "centerpiece of French scientific practice" until the 1710s (p. 58). Yet, if "centerpiece" it was, then French "scientific practice" must have been in a parlous state indeed, even within the Académie des Sciences. For contemporaries recognized that, owing to a dearth of local talent, French analysts lagged behind savants in Germany, Italy, and England. In 1709 Rémond de Montmort, for one, ascribed the poor state of the mathematical sciences in France to Louis XIV's wars, while a decade later he bitterly lamented France's disgraceful inability to raise anyone "capable of entering the lists with the English and the Germans."[3] This, in turn, leads Shank to misrepresent the Académie's history during the early eighteenth century. He is wrong to assert that the analysts wielded significant influence over its affairs until 1710; consequently, his claims regarding their deliberate marginalization thereafter lack evidentiary support.

If the dearth of analytical mechanists undermines Shank's argument that the *Principia* was quickly, if peculiarly, assimilated into French scientific culture, its corollary—that Newton's *physics* proper was ignored until the late 1720s—is equally weak. Shank repeatedly denies the existence of any Cartesian/Newtonian polarization prior to its "invention" by Fontenelle and Voltaire because, he asserts, the *Principia* was simply not recognized as a book of physics but was instead thought of as a work in mechanics with

[1] Niccolò Guicciardini, *Reading the* Principia*: The Debate on Newton's Mathematical Methods for Natural Philosophy from 1686 to 1736* (Cambridge: Cambridge Univ. Press, 1999), pp. 201–205; and Michel Blay, *La naissance de la mécanique analytique: La science du mouvement au tournant des XVIIe et XVIIIe siècles* (Paris: Presses Univ. France, 1992).

[2] Domenico Bertoloni Meli's astute deconstruction of Leibniz's *Tentamen* shows just how vexed and vexing the encounter could be; see Meli, *Equivalence and Priority: Newton versus Leibniz: Including Leibniz's Unpublished Manuscripts on the Principia* (Oxford: Clarendon, 1993).

[3] John L. Greenberg, *The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science* (Cambridge: Cambridge Univ. Press, 1995), p. 243; and Lenore Feigenbaum, "The Fragmentation of the European Mathematical Community," in *The Investigation of Difficult Things: Essays on Newton and the History of the Exact Sciences*, ed. P. M. Harman and Alan E. Shapiro (Cambridge: Cambridge Univ. Press, 1992), pp. 383–397, on p. 387.

several peculiarities that nevertheless did not raise any particularly pressing issues of physical understanding. French public discourse on science, he writes, regarded Newton as a "noteworthy but easily dismissed figure," which explains why "so little discussion of Newtonian science either for or against" existed earlier (p. 46). This claim can only be sustained through a selective reading of sources that eliminates the potency of the early opposition to Newton. The immediate and profound impact exerted by the *Principia* on European savants rendered it unnecessary to mention Newton by name when controverting him. Starting in the late 1680s, with Huygens's fervent search for more secure foundations upon which to restore the vortical system that Newton destroyed—and with Leibniz's determination to erect a planetary theory that bested Newton's—the *Principia* became the foil against which investigations into celestial mechanics, as well as terrestrial physics, were carried out. Shank misses the point because he focuses primarily on prefaces to periodical literature, to the detriment of the content of the science therein discussed.

Constraints of space prevent an elaboration of the extent to which the *Principia* informed European science between 1690 and 1715, not only in the science of Huygens and Leibniz but in that of Malebranche. However, since the Académie des Sciences is the focal point of *The Newton Wars*, it is appropriate to emphasize here Newton's omnipresence in the investigations carried out by its members. Consider Giovanni Domenico Cassini. As early as August 1690, during a visit of the deposed James II to the Observatory, he and his fellow astronomers disputed Newton's position on the effect of gravity on the earth's shape. The following year Johann Caspar Eisenschmidt, the Strasbourg mathematician and collaborator of Cassini, openly challenged Newton on the very same subject. He claimed that geodesic observations demonstrated that the length of the degree of the meridian decreases as the latitude increases; hence the globe must be elongated at its poles and not, as required by the *Principia*, shortened.[4] The topic remained contested. In November 1701 Cassini presented new meridian measurements at a public meeting of the Académie that corroborated Eisenschmidt's claim. Though he noted the divergence of Eisenschmidt's claim from the (otherwise quite different) theories expounded by Huygens and Newton, Cassini refrained from openly aligning himself with Eisenschmidt. Fontenelle, however, evidently concluded that the observations invalidated Newton's theory, and in summarizing the lecture for the *Histoire* he waxed eloquent on philosophy's good fortune "that a consequence derived from such a great number of completely new principles, based on the Cartesian hypothesis of gravity, and on the subtle Geometry of central forces, is found in such perfect agreement with actual and indubitable measurement." Unfortunately for Fontenelle, he deduced the wrong conclusion from the measurements, as if they demonstrated a globe *flattened* at its poles—a far from innocuous error, for which he was vigorously criticized by Maupertuis years later.[5]

Shank does not consider this episode, which is hardly consistent with his claim that at the time there was little French interest in salvaging the Cartesian heavens from the shipwreck brought about by the *Principia*. Nor does he note Cassini's related efforts to undermine the physics of the *Principia*. Cassini's devotion to Descartes manifested itself in his rejection of a finite speed of light—despite his being the first to deduce it from observations!—and in his semi-Cartesian theory of comets. True, Cassini's commitment to the instantaneous propagation of light and to his cometary theory predated the *Principia*.

[4] Johann Caspar Eisenschmidt, *Diatribe de figura telluris elliptico-sphaeroide* (Strasbourg, 1691).

[5] David Beeson, *Maupertuis: An Intellectual Biography* (Oxford: Voltaire Foundation, 1992), p. 54. The embarrassing error necessitated furtive rewriting of the paragraph when the volume was reprinted.

Yet he never wavered on either issue, even after perusing the copy that Halley had given him. If anything, Cassini's resolve to undermine Newton's cosmology only intensified. Thus in 1693 he publicly challenged the notion that Keplerian ellipses truly represent planetary motion, positing instead oval orbits wherein the *product*, and not the *sum*, of planetary distances to two fixed points is constant.[6] Newton was not mentioned, but the implications for the validity of key theorems in the *Principia* demonstrating and extending Kepler's laws were unmistakable. Leibniz, for one, was encouraged to contemplate discarding ellipses, too, in his effort to sustain a vorticist theory, and he eagerly solicited clarifications on Cassini's position.[7]

In the case of Cassini's covert anti-Newtonian campaign, Shank has not grappled with countervailing evidence to his general denial of an early Cartesian/Newtonian polarization. Elsewhere it is his use of the evidence that raises questions. Consider his representation of Fontenelle's role in the Newton wars. Cognizant of the Secretary's "deep epistemological commitment to Cartesian science," and of his endeavor since 1700 to render "vortical mechanics a centerpiece of his public academic discourse," Shank nevertheless asserts that no anti-Newtonian "polemical animosity" informed Fontenelle's public discourses before 1728. Such a stance was superfluous, since, he argues, no "precise philosophical position or sect of philosophers opposed to Descartes" existed either in France or "anywhere else in Europe" before 1715 at the earliest (pp. 69, 67, 47). Universal gravitation, Shank thinks, simply lacked the urgency and divisiveness that characterized it subsequently, when new historical contingencies engendered acrimonious controversies.

Here and in a recent article on Fontenelle's "alleged Cartesianism," to which readers are referred, the Secretary is depicted as "one of France's first 'Newtonians,'" entirely on the basis, it seems, of Shank's conflation of Fontenelle's embrace of the calculus with an embrace of analytical mechanics. There is little, if any, evidence for this. Equally to the point, Fontenelle's commitment to vorticist cosmology is alleged to have been "anything but dogmatic," merely furnishing the urbane and probabilist Secretary with an elegant picture—one of many possible pictures—with which to illustrate the workings of nature. For Fontenelle and his compatriots, Shank contends, there existed no necessary connection between fidelity to Descartes and opposition to Newton.[8] A careful reading of Fontenelle's works cannot sustain such an interpretation. Fontenelle's centrality to Shank's narrative consequently warrants an extended illustration of his quite early anti-Newtonian crusade.

By necessity, Newton's position as a foreign associate of the Académie prior to 1727

[6] Giovanni Domenico Cassini, *De l'origine et du progrès de l'astronomie* (Paris, 1693), rpt. in *Oeuvres diverses de M. I. D. Cassini* (Paris, 1730), pp. 43–44. For Cassini's continuing commitment to his Cartesian views see Laurence Bobis and James Lequeux, "Cassini, Rømer, and the Velocity of Light," *Journal of Astronomical History and Heritage*, 2008, *11*:97–105.

[7] Cassini's colleague, La Hire, also rejected ellipses and pronounced that planetary orbits failed to describe any perfectly regular curve. See G. W. Leibniz, *Mathematische Schriften*, ed. C. I. Gerhardt, 7 vols. (Halle: H. W. Schmidt, 1856), Vol. 2, Pt. 3, p. 498; Johan I Bernoulli, *Der Briefwechsel von Johann I Bernoulli*, Vol. 2: *Der Briefwechsel mit Pierre Varignon*, ed. Pierre Costabel and Jeanne Peiffer (Basel: Birkhäuser, 1988), Pt. 1, p. 182; I. Bernard Cohen, "Leibniz on Elliptical Orbits: As Seen in His Correspondence with the Académie Royale des Sciences in 1700," *Journal of the History of Medicine and Allied Sciences*, 1962, *17*:72–82; and Arthur Birembaut, Costabel, and Suzanne Delorme, "Les correspondance Leibniz–Fontenelle et les relations de Leibniz avex l'Académie royale des Sciences en 1700–1701," *Revue d'Histoire des Sciences et de Leurs Applications*, 1966, *19*:115–132.

[8] J. B. Shank, "On the Alleged Cartesianism of Fontenelle," *Archives Internationales d'Histoire des Sciences*, 2003, *53*:139–156.

mandated that, for reasons of propriety and order, any crusade against him had to be covert—a restriction that hardly diminished its potency. Case in point is the effective manipulation of the *Histoire*, already noted in the context of Cassini. In 1701, for example, the Académie launched an ambitious undertaking to collect tidal information. Shank denies that the effort had any connection to the *Principia*, ascribing it instead solely to a new utilitarian plan initiated by the government and celebrated by Fontenelle as a "worthwhile collaboration between the Crown, the Academy, and the public" (p. 65). Yet, no such celebration appears in Fontenelle's *Histoire*, save for gratitude for Pontchartrain's continued support of projects initiated by the academicians themselves. More disconcerting is Shank's failure to mention that Fontenelle openly conferred on Descartes "all honor" for having discovered the cause of tides and went on to rationalize the project as an effort to ascertain that the phenomenon would be comprehended with the same exactitude that Descartes's system already possessed.[9] Equally telling is the persistence with which Fontenelle routinely undermined the tenor of Varignon's contributions to the elucidation of central forces—a critical element of Newton's system—and his habitual framing of such work within the context of Cartesian vortices, a framing that was wholly absent from Varignon's analyses.

Fontenelle was also instrumental in the promotion of Philippe Villemot's *Nouveau système* (1707). He furnished the requisite imprimatur and most likely authored the lengthy and favorable review in the *Journal des Savants*, before situating the book at the center of the Académie's activities. The *Nouveau système* generated considerable excitement by virtue of being the first French attempt to reconcile vortices with Kepler's third law and related empirical observations—albeit without resort to higher mathematics. Villemot feigned ignorance of the *Principia* before completing his manuscript, but the book's argument clearly indicates that Newton's masterpiece had been his target.[10] Fontenelle used Villemot's book in order to forge a new context within which to discuss central forces—having argued during the previous five years, in an effort to play down the import of Varignon's work, that the topic had been exhausted—as well as Cassini's cometary theory. The excitement generated by the prospect of rectifying Cartesian celestial physics along the contours suggested by Villemot may well have led to the election on 12 March 1707 of three *élèves*—Bomie, Saulmon, and Saurin—who were expected to carry out such work and to challenge Newton directly.

Bomie was directed to work on central forces and further reconcile Cartesian vortices to Kepler's laws—the first fruits of which he delivered at a public meeting of the Académie on 18 April 1708. Saulmon was set to explore cylindrical vortices—both mathematically and experimentally—as part of researches that were presented in the early 1710s. Saurin proved the most aggressive. Aligning himself with the "ingenious" Villemot in a 1709 public lecture, Saurin challenged the validity of objections to vortices by both Huygens and Newton and boasted his triumph in locating "the cause of gravity in the centrifugal endeavor of the celestial matter that surrounds us." He concluded with a pointed jibe at Newton, who

> chooses rather to consider gravity as a quality inherent in bodies, and to renew the exploded notions of occult qualities and attraction. We must not flatter ourselves, that in all our physical

[9] *Histoire de l'Académie Royale des Sciences* [1701] (rpt., Paris, 1743), p. 11.

[10] Leibniz, similarly, feigned ignorance of the *Principia* in writing his *Tentamen*, though Meli has proved otherwise; see Meli, *Equivalence and Priority* (cit. n. 2).

> inquiries, we can ever surmount all difficulties: but however let us always philosophize upon clear, mechanical principles; if we quit them, all the light that we can have is extinguished, and we are plunged anew into the old darkness of peripateticism, from which heaven preserve us.[11]

Significantly, every work that challenged Newton was presented during one of the biannual open meetings of the Académie, thereby ensuring the widest possible public hearing. Fontenelle's commemorations of deceased members were delivered at these public meetings as well, and they, too, provided occasions to assail Newton. The 1715 eulogy of Malebranche, for instance, enabled the Secretary to marvel at the Oratorian's rectification and extension of Cartesian science. Above all, he extolled Malebranche's "key to the whole of physics"—the principle of minute vortices—from which gravity itself, hitherto "incomprehensible," followed. When eulogizing Montmort four years later, Fontenelle noted with evident glee how neither his election as a Fellow of the Royal Society nor his friendship with Brooke Taylor seduced Montmort to embrace universal gravitation. He remained a steadfast defender of Cartesianism and "sent the Attractions back to the nothingness from which they were trying to emerge." And rightly so, Fontenelle concluded. For "if one wishes to understand what is said, there are only Impulsions, and if one does not care to understand, there are Attractions, and anything else one likes."[12]

The powerful abbé Bignon, who regularly served as President of the Académie des Sciences, evidently shared Fontenelle's prejudice in favor of vortices. As one of Bignon's eulogizers put it, "Descartes's system, which was dominant when he was a student, received all his admiration; vortices were always dear to him. He was too confident to be in the least unfaithful to them in favor of Newtonian attraction, so fashionable today."[13] Shank does not examine the considerable body of evidence documenting the Académie's tenacious anti-Newtonian campaign or Fontenelle's orchestration—undoubtedly with Bignon's approval—of its public articulation. Saurin's lecture, for example, is noted in passing for breaking "the official silence surrounding" universal gravitation, while Montmort's eloge is misleadingly stated to exemplify Fontenelle's "*hônnéte* management" of disputes (pp. 125, 147). There was little if anything "*hônnéte*" about Fontenelle's literary ploys, and to assert otherwise ignores the ample evidence for his characteristic manipulations. Nor does Shank explore the much larger body of evidence attesting to the pervasive opposition to Newtonian science by the educated public at large.

The absence of countervailing evidence from Shank's account, or its downplaying, reflects one of its primary aims, which is less to present a thoroughgoing picture of the early diffusion of Newtonianism into France than to develop a rather tendentious argument regarding self-fashioning—an argument that would at best be extremely difficult to make if the true extent of French opposition to Newton had been presented—if, in other words, the broad outlines of the traditional picture hold true. To debate whether the imprecise notion of "self-fashioning" is a useful historical device is beyond the scope of this essay,

[11] Joseph Saurin, "D'une difficulté considerable proposée par M. Hughens contre le Systême Cartesien sur la cause de la Pesanteur," *Mémoires de l'Académie Royale des Sciences* [1709] (rpt., Paris, 1733), pp. 131–148, on pp. 132, 146, 148.

[12] *Histoire de l'Académie Royale des Sciences* [1719] (Paris, 1721), p. 91; Bernard le Bovier de Fontenelle, *Oeuvres Complètes*, ed. Alain Niderst, 9 vols. (Paris: Fayard, 1994), Vol. 6, pp. 355, 474; and Charles B. Paul, *Science and Immortality: The Eloges of the Paris Academy of Sciences (1699–1791)* (Berkeley: Univ. California Press, 1980), p. 30.

[13] *Journal des Sçavans*, Aug. 1745, pp. 1430–1431.

but the related assertion that France was devoid of critical public opinion before the 1730s demands response. Though Shank acknowledges in passing the contentiousness of the Republic of Letters, he nevertheless ascribes a staunchly adhered-to code of civility to pre-Enlightenment savants. Their science was always "*honnête*"; "contestation and dispute were anathema and partisan philosophical warfare almost nonexistent"; "bellicose tendencies were anything but positive values within the *honnête* climate of science"; courtesy was prized over the "pursuit of intellectual conviction" (pp. 29, 62, 85, 105–106, 385). These are but a few instances of Shank's efforts to differentiate the earlier period from the Enlightenment proper, when alone a "new individualistic, critical, and argumentative *esprit*" came to the fore.[14]

In representing a pre-Enlightenment as a "polite" Republic of Letters, Shank expands on the problematic claims of Steven Shapin and Anne Goldgar. As Goldgar summarized the position, "for the sake, not of truth, but of unity, scholars glossed over" all issues that divided them. Indeed, even when "tempers ran high and salvation itself seemed to be at stake, politeness, and consequently the integrity of the learned community, often seemed even more important. Moderation was the only way to maintain that integrity."[15] Such claims concerning the purported civility of the Republic of Letters cannot be sustained in the light of documentary evidence that runs directly to the contrary. Far from privileging gentlemanly conduct and valuing consensus over truth, contemporaries forcefully articulated their belief in the propriety, even necessity, of controversies for the welfare of the community. Pierre Bayle was explicit on the matter. The Republic of Letters

> is an extremely free state. One recognizes there only the imperium of truth and reason; and, beneath their auspices, one wages war innocently against anyone else. There friends must guard themselves against their friends, fathers against children, step-fathers against their step-children. . . . There each is sovereign and judge of each. Here the laws of society do not impede the independence of the state of nature with respect to error and ignorance; all citizens have, in this respect, the right of the sword and can exercise it without requesting the permission of those who govern.

What was prohibited was not controversy but ridicule, "because satires tend to divest a man of his honor, which is a sort of civil homicide." "Though one may lose some reputation as a learned man, and perhaps some Pecuniary Profit," "if it be done in Support of the Cause of Reason, and for the Interest of Truth only, and in a civil manner, no Body ought to find Fault with it."[16]

The injunction to carry on disputes with civility did not mean that Bayle advocated a polite Republic of Letters; he merely proscribed ridicule and the use of abusive language. Bayle's own polemics fully attest to his true sentiments on the matter, sentiments that were fully shared by contemporaries. Consider the case of the Dutch foreign academician Nicolaas Hartsoeker—whom Shank does not mention. From the 1690s until his death in

[14] Nearly one hundred occurrences of "*honnête*" and its cognates appear in the book.

[15] Anne Goldgar, *Impolite Learning: Conduct and Community in the Republic of Letters* (New Haven/London: Yale Univ. Press, 1995), p. 217; see also Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago/London: Univ. Chicago Press, 1994). For a rejoinder see Mordechai Feingold, "When Facts Matter," *Isis*, 1996, *87*:131–139. For a recent analysis of the Republic of Letters and Enlightenment sociability see Antoine Lilti, *Le monde de salons: Sociabilité et mondanité à Paris au XVIIIe siècle* (Paris: Fayard, 2005).

[16] *The Dictionary, Historical and Critical, of Mr Peter Bayle*, 2nd ed., revised by Pierre Des Maizeaux, 5 vols. (London, 1735; rpt., New York: Garland, 1984), Vol. 2, p. 389, note D.

1725, he censured most living natural philosophers, Newton included, with little regard for decorum. He was unrepentant for the harshness of his criticism:

> I very humbly beg of all whose opinions I have attacked, perhaps with too much liberty, not to take it in a bad way, since I have most often done this only to invite them to do the same for mine, and to attract illustrious adversaries in this way, in order to increasingly discover by these means the truth, which is the only thing I seek. They can then make use of the right of reprisal, and in turn attack my opinions, which I abandon to them completely. This philosophical war will likely cost a bit of ink, but there will be no spilling of blood.

Johann Mencken evidently had someone like Hartsoeker in mind when quipping that what "is called liberty in the world of letters" is a euphemism for "unlimited freedom to attack and slander others on the least provocation." Nevertheless, he quickly proceeded to enumerate several early modern instances of animated controversy before concluding with a paean to them: the arts and the sciences "can never endure in a republic of letters which knows fear and servitude. No one can tell how much good the disputes among the learned do in stimulating genius and enhancing the life of learning." Firmin Abauzit concurred, deeming that controversies, including insults, prevented intellectual lethargy and ensured that truth emerged victorious; Dortus de Mairan boasted that "he was never bitter, but only hot in the pursuit of truth as he saw it"; D'Alembert noted that "anarchy, which destroys political states, on the contrary supports and ensures the subsistence of the republic of letters."[17]

Statements of this sort can be multiplied. They suggest that Republicans of Letters believed that the republic was far more resilient in the face of internecine strife than its biographers assume. Indeed, controversies were not just to be suffered; they were, rather, to be countenanced, as they ensured the preservation of the critical stance crucial for the growth of knowledge. Consequently, the pursuit of truth trumped politeness. The Académie des Sciences followed a similar understanding, and Shank is simply wrong to assert otherwise. He interprets the resolution of the calculus disputes in 1707 as indicative of harmony and consensual peace imposed on the academicians in accordance with its rules (p. 63). Yet these rules hardly proscribed disputes. The sole stipulation was that when debates arose among members, care should be taken to respect opponents and avoid "expressions of anger or contempt." A wider net than Shank's would have shown that debates were endemic to the Académie and, more importantly, were never considered harmful. Malebranche's numerous controversies nicely illumine the point, as do the even more numerous (and malevolent) disputes engaged in by the Bernoullis. Similarly, the controversy between Daniel Tauvry and Jean Mery over the motion of the blood in the fetus proved so fierce that Fontenelle attributed Tauvry's premature death to the vigor with which he'd defended himself. Indeed, Martin Lister, who visited Paris in 1698, recorded that this "great Breach" prevented him from conversing with either anatomist. He hoped, however, that some good would come of such "honest Emulation."[18]

Fontenelle often articulated the Académie's positive stance on vigorous debate. As early as

[17] Nicolaas Hartsoeker, *Eclaircissemens sur les conjectures physiques* (Amsterdam, 1710), sig. *3; Johann Burkhard Mencken, *The Charlatanry of the Learned*, trans. Francis E. Litz (New York: Knopf, 1937), pp. 91–95; Ellen McNiven Hine, *Jean-Jacques Dortous de Mairan and the Geneva Connection: Scientific Networking in the Eighteenth Century* (Oxford: Voltaire Foundation, 1996), pp. 53, 108–109; and Jean le Rond d'Alembert, "Essai sur la société des gens de lettres et des grands," in *Oeuvres de D'Alembert* (Paris, 1822), Vol. 4, Pt. 1, p. 345.

[18] Fontenelle, *Oeuvres Complètes* (cit. n. 12), Vol. 7, p. 76; and Martin Lister, *A Journey to Paris in the Year*

1701, he pointed out that nothing contributed more to the advancement of the sciences than "emulation among the learned," within certain bounds. For this reason the Académie had differentiated its proceedings from those common in the schools, where the goal was not truth but to "avoid being reduced to silence." The academicians, in contrast, proceeded "without ostentation of ingenuity or knowledge"; none was "obliged to be in the right"; and it was always possible to yield without losing credit. Six years later, as a controversy between Lémery and Geoffroy unfolded, he commented on the value of generating disputes in the Académie: "The specific interest to prove that which one thinks, animates and heats up the love we have in general for truth." Fontenelle elaborated further on the matter in his hostile eloge of Antoine Parent, whose innate impetuousness, the Secretary believed, engendered a hypercritical temper that expressed itself "sometimes rashly and often without restraint." However, Fontenelle objected only to Parent's personal invective. The search for truth, he stated, mandated the provision of the freedom of contradiction. Parent transgressed merely in failing to recognize the necessity to respect those whom he contradicted.[19]

Shank's model Republican of Letters was the Lausanne pedagogue Jean-Pierre de Crousaz. He is depicted as a consummate *honnête homme*, cultivating "gentlemanly polymathy" without specialization, the likes of whom allegedly dominated the learned community. Shank fashions an apposite *persona* by combining adroitly chosen phrases without regard to context. The correspondence between Crousaz and the abbé Bignon is a case in point. Contrary to what Shank implies, this was not a free exchange of ideas between equals. It was a classic case of patronage relations involving an ambitious provincial savant and the most powerful man of letters in France. Consequently, Crousaz's grandiloquent expressions regarding his work, the language employed to solicit advice and guidance, his offers to dedicate books to Bignon, and so forth—which were naturally reciprocated politely—cannot be taken literally as reflective of a polite ideal of conduct shared by the community at large. The correspondence with Bignon, and then Réaumur, makes it clear that Crousaz tirelessly marketed himself. By late 1717 he openly solicited appointment as foreign associate of the Académie—to replace Leibniz. Rebuffed, he tried again six months later, acting on an unfounded rumor of Newton's death.

In a similar vein, consider Shank's contrast of the "judicious philosophical reasoning" and abhorrence of "passionate, sectarian theorizing" of Crousaz with the aggressive partisanship of the Newtonians. Shank asserts that a brief section in the *Traité du Beau* (1714), wherein Crousaz likened universal gravitation to "false beauty," rendered him odious to the English, who, consequently, rebuffed the polite efforts of the Swiss to communicate. Yet what prompted Crousaz to approach John Theophilus Desaguliers, Curator of Experiment to the Royal Society, in 1718 was his ambition to be elected a Fellow, a procedure that—Bignon and others informed him—required submission of written work. Furthermore, the English were irked not by Crousaz's *Traité du Beau* but by his submission of an inane attack on Newton's optical experiments (which even Bernoulli thought misguided) to the very person who in 1715 had successfully repeated such experiments before a delegation of the Académie des Sciences! The haughty and opinionated Crousaz was, in fact, a passionate and unwavering partisan of Cartesian science—who in 1772 announced a projected treatise aimed at "demolishing" Newton's

1698 (London, 1699), p. 67. "Emulation," a term often used by Fontenelle, was a common euphemism for "controversy."

[19] Fontenelle, *Oeuvres Complètes*, Vol. 7, pp. 353, 373–374; and *Histoire de l'Académie Royale des Sciences* [1707] (rpt., Paris, 1730), p. 43.

physics—and there was nothing polite in the persistence with which he pursued the campaign against Newton's experiments, despite their confirmation by Dortus de Mairan and Father Sebastian.[20] Clearly, then, to characterize Desaguliers's failure to respond to Crousaz as "disgraceful" and to insist, here and elsewhere, that the aggressive and partisan Englishmen violated the decorum of the Republic of Letters and threatened its harmony with discord cannot be squared with the evidentiary record (pp. 70–71, 159–160).

To address most claims of *The Newton Wars* would require a book of almost equal length. There are many errors of fact, both large and small. Giovanni Domenico Cassini came to France in 1669, not 1671, and his son's name was Jacques, not Jean-Dominique (p. 65); Malebranche's *Recherche de la verité* was first published in 1674, not in 1671 (p. 57); Louis XV was the great grandson, not the grandson, of Louis XIV (p. 87); Newton did not correspond with Richard Bentley in 1698 but in 1692/3 (p. 112); the Boyle Lectures comprised eight, not ten, sermons (p. 129); Catherine Barton was Newton's niece, not his sister (p. 259), and so on. There are places that verge on fiction. Shank, for example, avers that Montmort's "love affair with his future wife" "features prominently" in Montmort's early letters to Brooke Taylor (p. 134). Never mind that by then Montmort had been happily married for a decade to his one and only wife; Shank simply misinterpreted the letters' content. In one, Montmort effusively expressed veneration for Catherine Barton's beauty and intellect; in another, he attributed his inability to study to the prolonged visit that two charming young women paid his wife.[21] Or consider the discussion of Montmort's acquaintance, one de Rostaine, who was charged with delivering a package to Taylor. Although Shank concedes his inability to identify the man, he nevertheless asserts Rostaine's likely ties to Dutch publishers as well as his association with Parisian "progressive" circles (pp. 135–136).

These examples may seem innocuous enough, but they are symptomatic of the malleability of the evidentiary record in Shank's hands. Noteworthy is his claim that Newton's early French reception was tarnished by heretical connotations, Spinozism in particular. A factual statement by a reviewer of David Gregory's *Astronomiae physicae & geometricae elementa* in the *Journal de Trévoux*—that since attraction is, so to speak, the "soul" of the Newtonian universe, it was necessary to discuss it first—is interpreted as follows: To call attraction "the soul of the universe" was tantamount to connecting universal gravitation "with the most threatening of all early eighteenth-century heretical philosophies: Spinozist pantheism" (p. 126).[22] There follows a nearly five-page digression on John Toland and French clandestine literature, intended to establish an unsupported claim: that "pantheist discourse about Newtonianism" was widespread and that Frenchmen grew accustomed from the start to associate Newtonianism with debates about radical metaphysics and theology (p. 131).

Likewise, Shank mischaracterizes the *Trévoux*'s review of William Whiston's *Praelectiones astronomicae* in the following year. The reviewer, he writes, quickly went on the attack. Claiming "to hear Epicurus singing throughout Whiston's treatise," he "cited a Latin passage celebrating the all-pervasive *vis animus* as a representative synopsis of the work" (pp. 131–132). Shank mistakes the reviewer's entirely clear meaning. This was not

[20] Jacqueline E. de La Harpe, *Jean-Pierre de Crousaz et le conflit des idées au siècle des lumières* (Geneva: Droz, 1955), p. 238; and Abby R. Kleinbaum, "Jean Jacques Dortus de Mairan (1678–1771): A Study of an Enlightenment Scientist" (Ph.D. diss., Columbia Univ., 1970), p. 151.

[21] Brooke Taylor, *Contemplatio philosophica*, ed. William Young (London, 1793), pp. 88–89, 93–95.

[22] *Journal de Trévoux*, Feb. 1710, pp. 253–254.

to link Whiston to Epicurus but, rather, to quip that Whiston's veneration of Newton brought to mind Lucretius's tribute to Epicurus in *De rerum natura*: "Therefore the living force of his soul gained the day: on he passed far beyond the flaming walls of the world and traversed throughout in mind and spirit the immeasurable universe; whence he returns a conqueror." The reviewer may have been snide, but he certainly made no attempt to insinuate Spinozism.[23] This is not to say that Newtonianism was never associated with Spinozist tendencies; only that such charges, few and far between, belonged to a later date and thus surely did not help set the context for Newton's reception in France. In fact, Cartesianism lent itself to charges of Spinozism and religious heterodoxy far more readily than Newtonianism ever did.

My focus on the first half of *The Newton Wars* is only in part motivated by considerations of space. I felt it necessary to demonstrate that the foundation that sustains the subsequent discussion of Voltaire, Maupertuis, and the French Enlightenment is extremely weak. As noted above, Shank asserts that "France absorbed Newton's science immediately and in substantial, if idiosyncratic, ways from as early as 1690." However, by "idiosyncratic" he means an exclusively mathematical treatment of motion, while "France" can at best stand for a handful of people. This focus on a few members of the Académie des Sciences, together with the adoption of a modish line of argument, distorts the French reception of Newtonianism and the history of the Académie's ties with it. The sharp distinctions that Shank has drawn between public and academic science did not exist, and partisan Cartesian opposition to Newton's physics was as potent in 1700 as it was three decades later. Nor was the European Republic of Letters governed by a code of conduct different in the early decades of the eighteenth century than subsequently. There may have been only one Voltaire, but long before he came onto the scene the Republic of Letters was inhabited by numerous fierce polemicists and a responsive public. Whatever differences existed were of degree, not kind—with the exception that by 1750 public articulation of heterodox *religious* opinions became the new reality.

At the outset Shank announces that *The Newton Wars* pierces the mythology—created by D'Alembert (and Condorcet)—that elevated Newton to iconic status in the *philosophe* movement and ascribed to French Cartesians an obstinacy that it would require Voltaire and Maupertuis to eradicate. *The Newton Wars* does not sustain this doubtful claim, which would in any case require what is notably absent here: a serious engagement with the wide range of issues that impinge on the diffusion of Newtonianism and on the origins of the Enlightenment. One is left to ponder the effects of fashionable trends on sound scholarship and to wonder why none of those who should have known better spoke up.

[23] *Ibid.*, July 1711, p. 1264; and Lucretius, *De rerum natura*, trans. H. A. J. Munro (London: Bell, 1903), pp. 131–132.